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ILLUSIONS.*

In reality this work might have been styled an essay on error, for the author deals, in his clear and masterly way, with other errors of the human judgment than those which are termed illusions in the narrower sense of that term. His essay loses nothing, and gains much by thus occupying a much broader field than the one, furnished by the sensory illusion, would constitute *per se*. Perhaps the most unfortunate part of the work, is the opening passage: "Common sense, knowing nothing of fine distinctions, is wont to draw a sharp line between the region of illusion and that of sane intelligence. To be the victim of an illusion is, in the popular judgment, to be excluded from the category of rational men. The term at once calls up images of stunted figures with ill-developed brains, half-witted creatures, hardly distinguishable from the admittedly insane. . . . . . The nineteenth century intelligence plumes itself on having got at the bottom of mediæval visions and church miracles, and it is wont to commiserate the feeble minds that are still subject to these self-deceptions."

We say this passage is an unfortunate one, and this particularly because of its position in the opening chapter of a book which, as we must particularly emphasize, is throughout one of the clearest and most readable psychological treatises that we have found in the English language; this passage on the other hand, is as full of wrong assumptions, misconstructions, and errors as a single paragraph can well be. The popular mind fails to contemn the bearer of an illusion, as it does the bearer of a delusion; the mediæval visions were not, even in popular parlance illusions, but hallucinations, and indeed the popular sense in which the term illusion is used, that is, the one employed by poets and classical writers, is anything but a reflection on the bearer of the illusion. The daydream, the poetic illusion, and the constructions of a sanguine temperament, are the objects associated in the lay-mind with that term.

On the fourth page is further evidence that the author has failed to discriminate practically between delusions, hallucinations, and illusions. After stating that alienists have good reason to limit the word illusion to illusory perceptions, he adds "such illusions of the senses are the most palpable and striking evidences of mental disease." Inasmuch as illusions are common with the sane, it is incorrect to lay greater stress on the not very frequent illusions of the insane, than on the marked and characteristic hallucinations and the still more universal delusions of that class.

The author defines an illusion as a species of error which counterfeits the form of immediate, self-evident, or intuitive knowledge whether as a sense perception or otherwise. Further on he discriminates between the illusion and the fallacy, by characterising the former as a falsification of primary or intuitive knowledge, and the latter as a falsification of secondary or inferential knowledge. It must be admitted that the author is happier in his discrimination than in his definition, and an illustration of the difficulty under which definers labor recurs in the peroration of the same chapter, where he says that the illusion is seen to arise through "some exceptional feature in the situation or condition of the individual, which, for the time, breaks the chain of intellectual solidarity which under ordinary circumstances binds the single member to the collective body." The greater portion of this passage would constitute an excellent nucleus for a definition of insanity, but at the same time it seems to us that it fails to cover those common illusions, which involve the visual apparatus, and of which familiar illustrations are furnished in most physiological text books. The dividing line between the delusion, the hallucination, and the illusion, should have been strictly drawn at the outset, by our author. We have offered the following as showing the difference between the hallucination and the illusion: While a hallucination is a subjective perception of an object as a real presence, without a real presence to justify the perception, and a memory is the subjective per-

* *Illusions, a psychological study*. By James Sully. New York, D. Appleton and Co.—Volume XXXIII. of the International Scientific Series.

ception of an object not actually present, involving the recognition of its absence, an illusion is the subjective perception of an object actually present, but in characters which the object does not really possess. With appropriate alterations these definitions will cover the abstract hallucination and phantastic illusion of Wundt as well.

In his second chapter, the author ably, but we believe unsuccessfully, endeavors to defend his refusal to recognize the distinction between illusion and hallucination as the leading principle of classification, though he admits the necessity of making this distinction in accordance with the leading alienists. Wundt, an authority whose teachings in psychological physiology the author of the present volume has most successfully assimilated, has drawn attention to the numerous connecting links existing between illusions and hallucinations, and yet strongly insists on utilizing their general differences as a basis of classification. We find the chief drawback to the otherwise great value of the work, in its failure to give adequate space to the anatomical mechanism concerned in false registrations of the perceptional and conceptional sphere. If it be borne in mind that while even hallucinations may be based on actual impressions, the latter are not the determining factor of the hallucination, the difficulty in discriminating between these perversions is overcome; this is illustrated by the occasional persistence of dream-images in the waking state, and the moving of certain hallucinated images consonant with the movements of the eye-ball. If an actual or subjective impression, say in the shape of *chromatopsia* and *tinnitus*, be granted to exist in a subject hallucinating the vision and voice of the Virgin Mary, it will be instantly recognized by every observant alienist, that the real determining factor is here centrifugal, while in the illusion, which constructs, out of a ball rolling in an ill-lighted apartment, a mouse, the determining factor takes a centripetal course. In the former instance, the misinterpretation lies ready made in the Cortex, and seizes on the slight external pretext, whose existence we only admit for the sake of the argument, to incorporate it, in its substance; in the latter, it is based upon an imperfect registration and a gradual constructive interpreting process. Nothing could more forcibly illustrate the correctness of these propositions than the very case cited from Wundt by Mr. Sully of a forester who saw the real objects of the outer world, (furniture and tapestry, for example,) through the wood piles which formed the subjects of his hallucinations.

With these remarks on the propositions of the opening chapters, our criticism ceases to be adverse. In the last twelve chapters of the book, the author gives a concise review of the chief theories held by alienists and metaphysicians on the perceptional illusion, the introspective illusion, dreams illusions of memory, and those of belief. We refrain from again pointing out places where the author encroaches on the fields of delusion and hallucination, as he has given a wider scope to his definition of the illusion, than we are inclined to consider proper. It is but just to say that he gives a just interpretation to the views of alienists, an interpretation which only occasionally manifests that tincture of uncertainty which is unavoidable on the part of one devoid of a practical knowledge of the insane.

The perusal of this work cannot fail to be profitable to the student of mental pathology as well as of metaphysics. More reliable in the latter field, than in the former, it is yet a successful attempt to present the modern German ideas on the subject, and to combine the teachings of the practical and the abstract psychologists. To the general reader we can only repeat, what we said at the outset, it is the clearest rendition of a difficult yet fascinating theme, to be found in our language.

E. C. SPITZKA, M. D.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. BURDON-SANDERSON, M.D., LL.D., F.R.S.

The two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shop of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of

which it is the effect or outcome? And (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labors. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e.g.* J. Müller—recognized that the knowledge they possessed, both of vital and physical phenomena, was insufficient to refer both to a common measure. The method, therefore, was to study the process of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly to the great physiologists still living and working among us, who were the pupils of J. Müller—viz.; Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise [1] on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discovery in physics, there can be no doubt, that as a physiologist, he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavor shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine in which fuel is continually being used on the one hand, and work is being done and heat produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely muscle. Although, he said, a muscles acts under the direction of the will, it does not derive its power of acting from the will, any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i. e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscles of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and reintegrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process, either in the whole animal body or in a single muscle, during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,[1] continued by Pettenkofer and Voit,[2] and by the agricultural physiologists[3] with reference to herbivora, they are not by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfaetorily worked out, but that the quantitative relations of heat production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel; ein Beitrag zur Naturkunde,' Heilbronn, 1845.

[1] Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig, 1852.

[2] Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866–80.

[3] Henneberg and Stohmann, "Beiträge zur Begründung einer rationellen Futterung der Wiederkäuer," Brunswick and Göttingen, 1860–70.

as certain as of any physical fact that the animal body in doing work does not use its own material— that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he is unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology —*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true "Oel in der Flamme des Lebens" is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately--has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendency, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—viz., first, the investigation of the properties of the coloring matter of the blood, since called "hæmoglobin," the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The color of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the "blood-pump," as it is called, we have learned a great many facts relating to the nature of respiration, particularly that the difference of venous arterial blood depends not on the presence of "effete matter," as used to be thought, but on the less amount of oxygen held by its coloring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.[1] But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learned that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i. e.*, to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localized oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all,

that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "Elementa Physiologiæ" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavored to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the provisions of J. R. Mayer, and that what we have learnt with so much labor by experiments on animals is but the realization of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the provisions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favorable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realization; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you today may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of diseases. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

METEORIC DUST.

By Prof. Schuster.

A committee of the British Association was appointed for the double purpose of examining the observations hitherto recorded on the subject of meteoric dust and of discussing the possibility of future more systematic investigations. With regard to the first point we note that in a paper presented to the Royal Astronomical Society in 1879, Mr. Ranyard has given what appears to be a pretty complete account of the known observations as to the presence of meteoric dust in the atmosphere. It appears that in the year 1852 Prof. Andrews found native iron in the basalt of the Giant's Causeway. Nordenskjöld found particles of iron which in all probability had a cosmic origin in the snows of Finland and in the ice-fields of the Arctic regions. Dr. T. L. Phipson, and more recently Tissandier, found similar particles deposited by the winds on plates exposed in different localities. Finally, Mr. John Murray discovered magnetic particles raised from deposits at the bottom of the sea by H. M. S. *Challenger*. These articles were examined by Prof. Alexander Herschel, who agreed with Mr. Murray in ascribing a cosmic origin to them. For fuller details and all references we must refer to Mr. Ranyard's paper. There cannot be any doubt that magnetic dust, which in all probability derives it origin from meteors, has often been observed, and the question arises, in what way we can increase our knowledge on these points to an appreciable extent. A further series of occasional observations would in all probability lead to no result of great value, unless they were carried on for a great length of time in suitable places. Meteoric dust, we know, does fall, and observations ought if possible to be directed rather towards an approximate estimate of the quantity which falls within a given time. Difficulties very likely will be found in the determination of the locality in which the observations should be conducted. The place ought to be sheltered as much as possible against any ordinary dust not of meteoric origin. The lonely spots best fitted for these observations are generally accessible to occasional experiments only, and do not lend themselves easily to a regular series of observations. Nevertheless experiments continued for a few months at some elevated spot in the Alps might lead to valuable results. The Committee would like to draw attention to an instrument which is well fitted for such observations. It was devised by Dr. Pierre Miquel for the purpose of examining, not the meteoric particles, but organic and organized matters floating about in the air. A description, with illustrations, will be found in the *Annuaire de Montsouris* for 1879. Two forms of the instrument are given. In the first form, which is only adapted to permanent places of observations, an aspirator draws a quantity of air through a fine hole. The air impinges on a plate coated with glycerine, which retains all solid matter. By means of this instrument we may determine the quantity of solid particles within a given volume of air.

The second, more portable form, does not allow such an accurate quantitative air analysis. The instrument is attached to a weathercock, and thus is always directed against the wind, which traverses it, and deposits, as in the other permanent form, its solid matter on a glycerine plate. An anemometer placed in the vicinity serves to give an approximate idea of the quantity of air which has passed through the apparatus. These instruments have been called aëroscopes by their inventor. It is likely that the second form given to the apparatus will be best fitted for the purpose which the Committee has in view.

THE NEW ASTRONOMER ROYAL.

Mr. William Henry Mahony Christie, who has succeeded Sir George Airy in the office of Astronomer Royal at the Royal Observatory, Greenwich Park, was born on October 1, 1845, at Woolwich. He is a younger son of the late Professor S. H. Christie, of the Royal Military Academy, Woolwich, and formerly Secretary to the Royal Society. Mr. W. H. M. Christie was educated at Kings College School, London; and at Trinity College, Cambridge, which he entered in 1864, having won a Minor's Scholarship of that College; he subsequently gained a Foundation Scholarship and was afterwards elected a Fellow of Trinity College. He took his degree of B.A. in 1868, as fourth wrangler in the Mathematical Tripos, and in 1871 proceeded to the M. A. degree. In 1870, Mr. Christie was appointed Chief Assistant at the Royal Observatory; and he has, during the past ten years, done special good service by contriving and introducing several valuable improvements in the scientific apparatus there in use; a new form of spectroscope, an instrument for determining the colors and brightness of the stars, a recording micrometer, and a polarising solar eye-piece, are to be mentioned as his inventions. In the recent address of the President of the British Association, at York, a passing reference was made to Mr. Christie's work in verifying the results obtained by Dr. Huggins, with regard to the motions of stars, as inferred from spectroscopic observations. The new Astronomer Royal has directed particular attention, at the Royal Observatory, both to spectroscopy and to photography, as a means of recording the observations. He is a fellow of the Royal Society, and was elected Secretary of the Royal Astronomical Society last year. He contributed to the proceedings of the Royal Society, in March, 1877, a paper "on the magnifying power of the half-prism, as a means of obtaining great dispersion, and on the general theory of the half-prism spectroscope." To the monthly notices of the Royal Astronomical Society, he has furnished these: in June, 1873, a paper on the recording micrometer; in January, 1874, on the color and brightness of stars, as measured with a new photometer; in May, 1875, on the determination of the scale in photographs of the Transit of Venus; in 1876, (January) on a new form of solar eye-piece; (May) on the displacement of lines in the spectra of stars; (November) on the effect of wear in the micrometer screws of the Greenwich Transit Circle; same year (December) on the gradation of light on the disk of Venus; in 1878 (January) on specular reflection from Venus; (June) on the existence of bright lines in the solar spectrum; in 1879 (January) on a phenomeno seen in the occultation of a star by the moon's bright limb; in 1880, November, on the spectrum of Hartwig's comet of that year; in 1881 (January) on Mr. Stone's alterations of Bessel's refractions; (May) on the flexure of the Greenwich transit circle, and some further remarks on Mr. Stone's alterations of Bessel's refractions; besides various papers on the Greenwich spectroscopic and photographic observations, communicated by the late Astronomer Royal; and a paper which will be found in the Memoirs of the Royal Astronomical Society, published in January, 1880, on the systematic errors of the Greenwich North Polar distances. Mr. Christie is also the founder and editor of a journal entitled "*The Observatory*, a Monthly Review of Astronomy," which has been published during the past four years; and he is author of the "Manual of Elementary Astronomy," published in 1875 by the Society for Promoting Christian Knowledge.

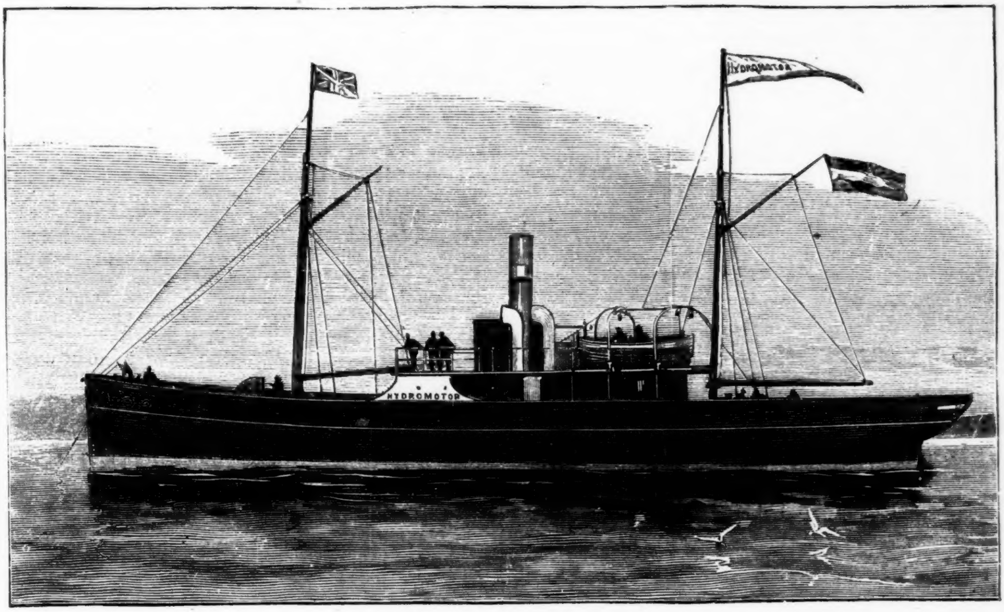

WILLIAM H. M. CHRISTIE.

ON THE ELECTRIC CONDUCTIVITY AND DICHROIC ABSORPTION OF TOURMALINE.*

By Prof. SILVANUS P. THOMPSON.

The electric conductivity of tourmaline differs in different directions; being, according to the author's experiments, a minimum along the optic axis. Tourmaline also possesses the optical property of dichroism, its absorption being a maximum for rays parallel to the axis, and greater for blue rays than for red, equal thicknesses of crystal being considered. According to the electro-magnetic theory of light, bodies which are good conductors of electricity should be opaque to light. The author has in the August number of the *Philosophical Magazine* rewritten the equations of Maxwell's electro-magnetic theory for the case of crystalline media possessing different conductivities in different directions. From these equations it appears that in tourmaline and negative uniaxial crystals electric displacements at right angles to the axis will be more absorbed than electric displacements parallel to the axis. This accounts for the well-known greater absorption of the ordinary ray, provided the views of Stokes and Fresnel are correct, that these displacements are at right angles to the so-called plane of polarization. The difference of velocity between the rays of different color accounts for the difference of absorption being greater in that direction in which the conductivity is a minimum. It was also pointed out that in positive uniaxial crystals, in which the electric conductivity is a maximum along the axis, there will be maximum absorption of the extraordinary ray, and there will be least opacity along the axis. Smoky quartz and magnesic platinocyanide fulfil the latter condition. Specimens of tourmaline cut into cubes to show the colors in different directions were shown, and also specimens of magnesic platinocyanide and of herapathite. Mechanico-optical models were also shown illustrating the theory; a tourmaline being represented by a cube built up of layers of glass and wire-gauze. In conclusion it was shown that crystals in which the electric conductivity differs in three different directions will exhibit *trichroism;* and that di- or trichroic absorption is a general property of all colored crystals other than those of the cubical system.

* British Association, 1881.

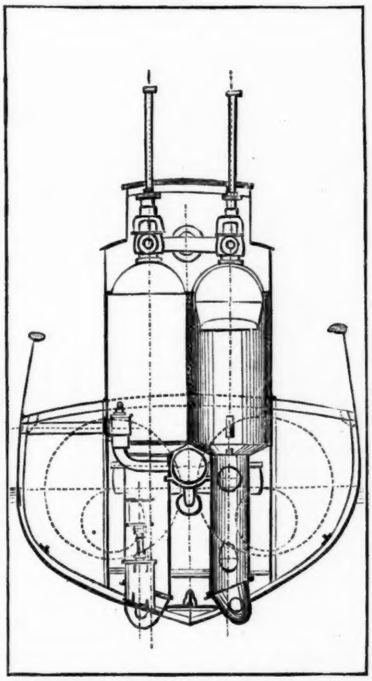


THE VESSEL AS IT APPEARS AFLOAT

THE HYDROMOTOR SHIP.

We present a drawing of a vessel propelled by hydraulic reaction, and recently constructed at Kiel by Dr. Emil Fleischer, of Dresden.

Machines propelled on the reactive principle are by no means a novelty, but hitherto have been attended with indifferent success. Nearly 40 years ago a model boat, which used to travel up and down a tank propelled by such means, was exhibited in London. But Dr. Fleischer's hydromotor allows of as much as 90 per cent. of the indicated steam power being applied to the production of the outflowing water stream, while not more than 30 per cent. has been secured with the reaction machines hitherto constructed. In his vessel the usual ship's engines, worked by means of wheel or screw, are replaced by hydraulic reaction, by the drawing in and shooting out of a stream of water. The steam power acts immediately on the water, without any loss of such power in conveyance from steam engines and pumps. The manœuvring capabilities of the vessel are greatly increased, and the usual complicated machinery is replaced by a remarkably simple contrivance.



The professional men who took part in a short trip with the hydromotor, expressed the most unqualified appreciation of the invention, and of every detail of its execution. The easy manœuvring of the vessel, its small consumption of coal, and the practicability of adapting the system to all rates of speed were clearly shown, and the simplicity of its construction was regarded as particularly valuable for war ships. The hydromotor is undergoing further tests in English waters.

HYDRODYNAMIC ANALOGIES TO ELECTRICITY AND MAGNETISM.

From a scientific and purely theoretical point of view there is no object in the whole of the Electrical Exhibition at Paris of greater interest than the remarkable collection of apparatus exhibited by Dr. C. A. Bjerknes, of Christiania, and intended to show the fundamental phenomena of electricity and magnetism by the analogous ones of hydrodynamics. I will try to give a clear account of these experiments and the apparatus employed; but no description can convey any idea of the wonderful beauty of the actual experiments, whilst the mechanism itself is also of most exquisite construction. Every result which is thus shown by experiment had been previously predicted by Prof. Bjerknes as the result of his mathematical investigations.

It has long been known that if a tuning-fork be struck and held near to a light object like a balloon it attracts it. This is an old experiment, and the theory of it has been worked out more than once. Among others, Sir William Thomson gave the theory in the *Philosophical Magazine* in 1867. In general words the explanation is that the air in the neighborhood of the tuning-fork is rarefied by the agitation which it experiences. Consequently the pressure of the air is greater as the distance from the tuning-fork increases. Thus the pressure on the far side of the balloon is greater than that on the near side, and the balloon is attracted.

Dr. Bjerknes has followed out the theory of this action until he has succeeded in illustrating most of the

fundamental phenomena of electricity and magnetism. He causes vibrations to take place in a trough of water about six inches deep. He uses a pair of cylinders fitted with pistons which are moved in and out by a gearing which regulates the length of stroke and also gives great rapidity. These cylinders simply act alternately as air-compressers and expanders, and they can be arranged so that both compress and both expand the air simultaneously, or in such a way that the one expands while the other compresses the air, and *vice versa*. These cylinders are connected by thin india-rubber tubing and fine metal pipes to the various instruments. A very simple experiment consists in communicating pulsations to a pair of tambours, and observing their mutual actions. They consist each of a ring of metal faced at both sides with india-rubber and connected by a tube with the air-cylinders. One of them is held in the hand; the other is mounted in the water in a manner which leaves it free to move. It is then found that if the pulsations are of the same kind, *i.e.* if both expand and both contract simultaneously, there is attraction. But if one expands while the other contracts, and *vice versa*, there is repulsion. In fact the phenomenon is the opposite of magnetical and electrical phenomena, for here like poles attract, and unlike poles repel.

Instead of having the pulsation of a drum we may use the oscillation of a sphere; and Dr. Bjerknes has mounted a beautiful piece of apparatus by which the compressions and expansions of air are used to cause a sphere to oscillate in the water. But in this case it must be noticed that opposite sides of ths sphere are in opposite phases. In fact the sphere might be expected to act like a magnet; and so it does. If two oscillating spheres be brought near each other, then, if they are both moving to and from each other at the same time, there is attraction; but if one of them be turned round, so that both spheres move in the same direction in their oscillations, then there is repulsion. If one of these spheres be mounted so as to be free to move about a vertical axis, it is found that when a second oscillating sphere is brought near to it, the one which is free turns round its axis and sets itself so that both spheres in their oscillations are approaching each other or receding simultaneously. Two oscillating spheres, mounted at the extremities of an arm, with freedom to move, behave with respect to another oscillating sphere exactly like a magnet in the neighborhood of another magnetic pole. I believe that these directive effects are perfectly new, both theoretically and experimentally. The professor mounts his rod with a sphere at each end in two ways: (1) so that the oscillations are along the arm, and (2) so that they are perpendicular. In all cases they behave as if each sphere was a little magnet with its axis lying along the direction of oscillalation.

Dr. Bjerknes looks upon the water in his trough as being the analogue of Faraday's medium; and he looks upon these attractions and repulsions as being due, not to the action of one body on the other, but to the mutual action of one body and the water in contact with it. Viewed in this light, his first experiment is equivalent to saying that if a vibrating or oscillating body have its motions in the same direction as the water, the body moves away from the centre of disturbance, but if in the opposite direction, towards it. This idea gives us the analogy of dia- and paramagnetism. If, in the neighborhood of a vibrating drum, we have a cork ball, retained under the water by a thread, the oscillations of the cork are greater than those of the water in contact with it, owing to its small mass, and are consequently *relatively* in the same direction. Accordingly we have repulsion, corresponding to diamagnetism. If, on the other hand, we hang in the water a ball which is heavier than water, its oscillations are not so great as that of the water in its vicinity, owing to its mass, and consequently the oscillations of the ball relatively to the water are in the opposite direction to those of the water itself, and there is attraction, corresponding to paramagnetism. A rod of cork and another of metal are suspended horizontally by threads in the trough. A vibrating drum is brought near to them; the cork rod sets itself equatorially, and the metal rod axially.

If a pellet of iron be floated by a cork on water and two similar poles (*e. g.* both north) be brought to its vicinity, one above and the other below the pellet, the latter cannot remain exactly in the centre, but will be repelled to a certain distance, beyond which, however, there is the usual attraction. The reason is that when the pellet is nearly in the line joining the two poles the north pole of the pellet (according to our supposition) is further from this line than the south one. The angle of action is less; so that although the north pole is further away, the horizontal component of the north pole repulsion may be greater than that of the south pole attraction. Dr. Bjerknes reproduces this experiment by causing two drums to pulsate in concord, the one above the other. A pellet fixed to a wire, which is attached by threads to two pieces of cork, is brought between the drums, and it is found impossible to cause it to remain in the centre.

Dr. Bjerkes conceived further the beautiful idea of tracing out the conditions of the vibrations of the water when acted upon by pulsating drums. For this purpose he mounted a sphere or cylinder on a thin spring and fixed a fine paint-brush to the top of it. This is put into the water. The vibrations are in most cases so small that they could not be detected, but by regulating the pulsations so as to be isochronous with the vibrations of the spring, a powerful vibration can be set up. When this is done a glass plate mounted on four springs is lowered so as to touch the paint-brush, and the direction of a hydrodynamic line of force is depicted. Thus the whole field is explored and different diagrams are obtained according to the nature of the pulsations. Using two drums pulsating concordantly, we get a figure exactly like that produced by iron filings in a field of two similar magnetic poles. If the pulsations are discordant it is like the figure with two dissimilar poles. Three pulsating drums give a figure identical with that produced by three magnetic poles. The professor had previously calculated that the effects ought to be identical, and I think the same might have been gathered from the formulæ in Sir William Thomson's "Mathematical Theory of Magnetism," but this only enhances the beauty of the experimental confirmation.

Physicists have been in the habit of looking upon magnetism as some kind of molecular rotation. According to the present view it is a rectilinear motion. Physicists have been accustomed to look upon the conception of an isolated magnetic pole as an impossibility, but here, while the oscillating sphere represents a magnetic molecule with north and south poles, the pulsating drum represents an isolated pole. These are new conceptions to the physicists, let us see whither they lead us. The professor shows that if a rectilinear oscillation constitutes magnetism, a circular oscillation must signify an electric current, the axis of oscillation being the direction of the current. According to this view what would be the action of a ring through which a current is passing? If the ring were horizontal the inner parts of the ring would all rise together and all fall together, they would vibrate and produce the same effect as the rectilinear vibrations of a magnet. This is the analogue of the Amperian currents.

To illustrate the condition of the magnetic field in the neighborhood of electric currents, Dr. Bjerknes mounted two wooden cylinders on vertical axes, connecting them by link-work, which enabled him to vibrate them in the same or opposite ways. To produce enough friction he was forced to employ syrup in place of water. The figures which are produced on the glass plate are in every case the same as those which are produced by iron filings in the neighborhood of electric currents, including the

case of currents going in parallel and in opposite directions.

The theory is carried out a step further to explain the attraction and subsequent repulsion after contact of an electrified and a neutral substance and the passage of a spark. But it is extremely speculative, and is not as yet experimentally illustrated, and I think that at present it is better to pass it by,

I believe that the professor will exhibit his experiments and give some account of his mathematical investigations, which have occupied his time for five years, to the Académie des Sciences this afternoon. His results have not been published before. GEORGE FORBES.

THE ELECTRIC DISCHARGE THROUGH COLZA OIL.*

By A. MACFARLANE, D. SC., F. R. S. E.

The electrical properties of colza oil which I have examined are its dielectric strength and some phenomena which accompany the passage of the spark. By the dielectric strength of a substance I mean the ratio of the difference of potential required to pass a spark through air under the same conditions. The electrodes used were two parallel brass plates each four inches in diameter. When comparing the gases the standard distance of the plate chosen was 5 mm. In the case of liquids it is convenient to observe for a shorter distance, and reduce the result by the law which previous experiments of mine have established, namely, that in the case of the discharge between parallel plates through a liquid dielectric, the difference of potential required is proportional to the distance between the plates. (*Trans.* R. S. E., vol. xxix. p. 563). One set of observations gave the ratio for colza oil to be 2.7, another gave 2.5. Hence 2.6 may be taken. I have now obtained the following table of dielectric strengths for liquids (1 being unity).

Substance.	Dielectric Strength.
Paraffine oil	3.7
Oil of turpentine...........	4.0
Paraffine liquefied..........	2.4
Olive oil...................	3.5
Colza oil...................	2.6

The specific gravity of the colza oil is .91. The passage of the spark was accompanied by the formation of gas bubbles, but there was no deposition of solid particles. As the 4-inch plates were placed horizontally in the oil a bubble produced by the discharge was prevented from escaping by the upper plate. When the upper plate is again electrified such a bubble behaves in the following manner. If it is large enough it will extend itself somewhat like an hour-glass between the plates, but if it is smaller it takes the form of an acorn with a flat base, the base resting on one or other of the plates. When the upper plate is charged positively the bubble is repelled so as to place its base on the lower plate; when the electricity is changed to negative the bubble remains with its base on the upper plate. A reversal of the order of charging did not change the effect. After a few electrifications a sufficient number of solid particles collect to form a chain, and thus interfere with the phenomenon, the bubbles then being lengthened out in a remarkable manner, but never repelled to the lower plate. When the upper plate was charged negatively, gas bubbles appeared to me to rise from the lower plate, as if they had been formed there. To test this point further I took some sparks between two smaller disks placed vertically in the oil. The gas-bubbles were observed to rise up at the negative surface as if they had been formed at the positive surface, and had been repelled or carried straight across, and then rose up at the negative surface. When the spark was taken between two points bent at right angles to two rods dipping into the oil, the bubbles were observed to shoot out in the direction from the positively charged point, and to circulate round the earth-rod some time before rising to the surface. These phenomena indicate that the bubble is positively electrified.

* British Association, 1881.

ASTRONOMY.

ON THE POSSIBILITY OF THE EXISTENCE OF INTRA-MERCURIAL PLANETS.*

By BALFOUR STEWART, LL.D., F.R.S.

It is a somewhat frequent speculation amongst those who are engaged in sun-spot research to regard the state of the solar surface as influenced in some way by the positions of the planets.

In order to verify this hypothesis observers have tried whether there appear to be solar periods exactly coinciding with certain well-known planetary periods. This method has been adopted by the Kew observers (Messrs. De La Rue, Stewart, and Loewy), who had an unusually large mass of material at their disposal, and they have obtained from it the following results:—

1. An apparent maximum and minimum of spotted area approximately corresponding in time to the perihelion and aphelion of Mercury.
2. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Mercury and Jupiter.
3. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Jupiter.
4. An apparent maximum and minimum of spotted area approximately corresponding in time to the conjunction and opposition of Venus and Mercury.

The Kew observers make the following remarks upon these results:—

"There appears to be a certain amount of likeness between the march of the numbers in the four periods which we have investigated, but we desire to record this rather as a result brought out by a certain specified method of treating the material at our disposal than as a fact from which we are at present prepared to draw conclusions. As the investigation of these and similar phenomena proceeds, it may be hoped that much light will be thrown upon the causes of sun-spot periodicity.

The Kew observers have likewise produced evidence of a different kind in favor of the planetary hypothesis, for they have detected a periodicity in the behavior of sun-spots with regard to increase and diminution apparently depending upon the positions of the two nearer planets, Mercury and Venus. The law seems to be that as a portion of the sun's surface is carried by rotation nearer to one of these two influential planets, there is a tendency for spots to become less and disappear, while on the other hand, when it is carried away from the neighborhood of one of these planets, there is a tendency for spots to break out and increase.

But whatever truth may be in these conclusions, it appears to be quite certain that periodical relations between the various *known* planets will not account for *all* the sun-spot inequalities with which we are acquainted. They may account for some, but certainly not for all. For there are solar inequalities of short duration which presuming them to be real, can only be accounted for on the planetary hypothesis, by supposing the existence of several unknown intra-Mercurial planets.

Indeed these short-period inequalities in sun-spots and the allied phenomena of terrestrial magnetism and meteorology have so augmented in number of late years as to make some observers inclined to question their reality; while others again resort to the above-mentioned hypothesis, and attribute them to intra-Mercurial planetary agency.

The method to be pursued in detecting the existence

* British Association, 1881.

of inequalities will be easily understood by an illustration. Suppose we had in our possession extensive records of the temperature of the earth's atmosphere at some one place in middle latitudes, and that, independently of astronomical knowledge, we were to make use of these for the purpose of investigating the natural inequalities of terrestrial temperature. We should begin by grouping the observations according to various periods taken, say, at small but definite time-intervals from each other. Now if our series of observations were sufficiently extensive, and if some of our various groupings together of this series should correspond to a real inequality, we should expect it to exhibit a well-defined and prominent fluctuation, whose departures above and below the mean should be of considerable amount.

Suppose, for instance, that we have twenty-four points in our series, and that we group a long series of temperature observations in rows of twenty-four each, the time distance between two contiguous members of one row being one hour. The series would thus represent the mean solar day, and we should, without doubt, obtain from a final summation of our rows a result exhibiting a prominent temperature fluctuation of a well-defined character, which we might measure (as long as we keep to twenty-four points) by simply adding together all the departures of its various points from the mean, whether these points lie above or below; in fine, by obtaining the area of the curve, which is the graphical representation of the inequality above and below the line of abscissæ taken to represent the mean of all the poiuts. Suppose next that, still keeping to rows of twenty-four, we should make the time interval between two contiguous members of a row somewhat different from one hour, whether greater or less, we should now in either case obtain a result exhibiting, when measured as above, a much smaller inequality than that given when the interval was exactly one hour; and it is even possible that, if our series of observations were sufficiently extensive, we should obtain hardly any traces of an inequality whatever.

In fine, when each row accurately represented a solar day, the result would be an inequality of large amount; but when each row represented a period either slightly less or greater than a day, the result would be an inequality of small amount. This process, as far as I have described it, is not new, inasmuch as something of this kind must be pursued in all attempts to detect inequalities. In the present instance we should by its means, after bestowing enormous labor in variously grouping, in accordance with a great number of periods taken at small intervals from each other, obtain definite results. These might be graphically represented in the following manner:—

The line of abscissæ might be taken to denote the exact values of the various periods, forming a time-scale, in fact, while the ordinates might represent the areas or summations obtained as above by employing these various periods. There would thus be in the case now used for illustration a very prominent peak, corresponding to twenty-four hours, which would fall off very rapidly on either side.

By means of the process now described we should at length, after enormous labor, obtain a graphical result, showing the exact position in the time-scale of the observed maximum inequality. In conjunction with Mr. William Dodgson, I have devised a method by which this labor is very greatly reduced, and the process so modified has been applied by us in order to determine whether there be inequalities of short period in the observed areas of the sun-spots occurring on the visible hemisphere of the sun. We have detected an inequality of this nature corresponding in period to 24.011 days, which, when subjected to a certain purifying treatment, appears to us to exhibit the marks of a true periodicity. But it has been suggested by Prof. Stokes that a method of this nature for detecting inequalities might with greater propriety be employed as a crucible for testing the value of some hypothesis introduced into it from without.

Acting upon this suggestion I have ventured to introduce the planetary hypothesis, and to ask whether the above sun-spot inequality of short period may not in reality be caused by an intra-Mercurial planet. It is quite easy to put this hypothesis to a test, taking for our guidance the results obtained by the Kew observers. For what do these results exhibit? In the first place they exhibit the probability of a sun-spot inequality corresponding to the period of Mercury round the sun; and in the next they exhibit the probability of similar inequalities corresponding to the synodic period of Mercury and Venus, and to the synodic period of Mercury and Jupiter.

Now if there be an intra-Mercurial planet of period 24.011 days, it will have the following synodic periods:—

With Mercury................33.025 days.
With Venus....... ... .26 884 days.
With Jupiter ....24.145 days.

In conjunction with Mr. Dodgson I have applied the above method of analysis with the view of ascertaining whether there be well-marked sun-spot inequalities nearly corresponding to these periods, and we have obtained the following results:—

A very prominent inequality of period......32.955 days.
A very prominent inequality of period......26.871 days.
A less prominent inequality of period... ..24.142 days.

It will thus be noticed that there are prominent sun-spot inequalities, the periods of which agree very well with the synodic periods of the supposed planet with Mercury, Venus, and Jupiter, more especially if we bear in mind that this is only a first approximation.

The test, however, is not yet complete. Referring once more to the results of the Kew observers, it will be noticed that we have approximately maxima of sun-spot areas when Mercury and Venus, or when Mercury and Jupiter are in conjunction. Now if we assume that there is an intra-Mercurial planet of period 24.011 days, we are as yet unable to assign its exact position in ecliptical longitude at any moment. We know its period, and we may presume that it has considerable eccentricity, but we know nothing else. We may, however, assume as most probable that the maximum point of the inequality of period 32.955 days corresponds to the conjunction of the planet with Mercury, the maximum point of the inequality of period 26.871 days to its conjunction with Venus, and the maximum point of the inequality of period 24.142 days to its conjunction with Jupiter. On this assumption, and knowing the average rate of motion of the planet in its orbit, we may deduce approximately its position at a given epoch independently from each of the three synodic periods above mentioned, and these positions ought to agree together, if our hypothesis be correct.

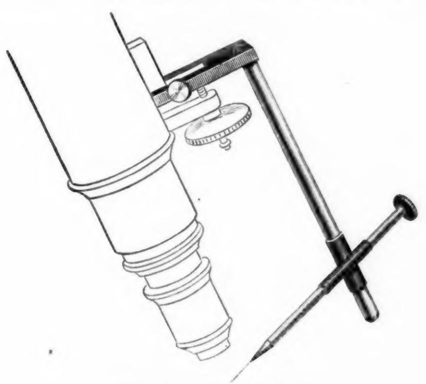
I have done this approximately, but am not able to bring exact figures before this meeting. The agreement is as great as can be expected, bearing in mind that we know only the average rate of motion of the planet, and not the variations of its rate, inasmuch as we are ignorant of its eccentricity. I think I may state that three independent values of its position corresponding to January 1, 1832, will be obtained, and that the mean difference of a single value from the mean of the whole will probably not be more than twenty degrees. It would thus appear from this investigation that the evidence is in favor of the sun-spot inequality of 24.011 days being due to an intra-Mercurial planet. Of course a single research of this nature is insufficient to establish a theory of this importance, but as there are several short-period solar inequalities, the same method may be pursued for each, an operation which demands nothing but time and labor. It appears to me of great importance that these short-period solar inequalities should be systematically examined after this method.

MICROSCOPY.

Mr. C. Henry Kain thus describes in the August number of *The American Journal of Microscopy*, his new mechanical finger for the microscope, which will be found a useful addition to the instrument.

A glance at the engraving will render the working of it intelligible to all. It consists essentially of a slotted bar which may be firmly clamped to the upper (immovable) bar of the fine adjustment by means of a milled-headed screw. Through the end of this is fastened a round rod, at such a distance from the objective that, when lowered, the end will not strike the stage. Over this rod slips a split tube, to which is soldered at an angle, a smaller tube. Through the small tube passes a rod carrying a glass hair at its extremity. This rod is easily rotated by means of a milled head. The capillary glass thread is attached to the extremity by means of beeswax. The arrangement of split tubes was suggested by Mr. Edward Pennock, to take the place of a binding screw which I had intended; it is a very neat and convenient affair, and much less clumsy than the arrangement I originally proposed. It will be noticed that the finger has no revolving collar, as it is quite unnecessary, especially when the microscope is provided with a revolving stage. By dispensing with the revolving collar and making all movements depend entirely upon the adjustments of the microscope, greater stability and accuracy in working are secured.

To use the finger, the point of the glass thread is first brought into the focus of the objective, or nearly so, by sliding the tube on the vertical rod and pushing or pulling the rod carrying the glass thread until the desired position is attained. It is not difficult to do this, and, having once been done by hand, it does not have to be repeated, as all further movements are made by the adjustments of the microscope. Supposing now the point of the glass thread



A NEW MECHANICAL FINGER.

to be in focus; by means of the fine adjustment throw the focus *ahead* of the point, then, by means of the coarse adjustment, rack down and search for the object you wish to pick up. Having found the object desired, again bring the point of the thread into focus by means of the fine adjustment; then rack down with the coarse adjustment and pick it up. Now rack back with the coarse adjustment, remove the slip on which the material is spread, and place your prepared slip or cover upon the stage. Again, by means of the fine adjustment, throw the focus ahead of the object, rack down with the coarse adjustment and search for the spot where you wish to deposit the object, and, having found it, again focus the object, then rack down with the coarse adjustment, and, when the object touches the slide and has been placed in proper position, fix it by means of a very gentle breath. There are many other devices by which this useful little instrument may be used for a variety of purposes, for a description of which we refer the reader to Professor Phin's journal.

PENNOCK'S OBLIQUE DIAPHRAGM.—The accompanying engravings show a new form of oblique diaphragm devised by Mr. E. Pennock, and described by him in *The American Journal of Microscopy* (August, 1881). It is designed to be attached to the under side of the stage for shutting off all light except a small pencil from the mirror. Its function is the same as Smith's >-shaped diaphragm. It is an adaptation of Mr. Mayall's spiral diaphragm, which was originally designed for use with condensers of wide aperture, and was described in a recent number of the *Journal of the Royal Microscopical Society*.

It may be mounted in either of two forms: the one to fit into the usual tube, which, in the cheaper microscopes, is attached to the under side of the stage; the other to screw directly into the stage aperture.

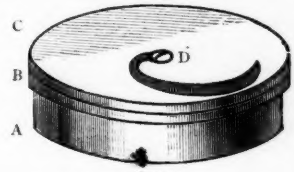


FIG. 1.

A. Tube 1½ inch in diameter, fitting into accessory tube beneath stage.

B. Upper plate (shown as under) having radial slot.

C. Under plate, having spinal slot.

D. Screw joining the plates.

The manner of using it to obtain pencils of varying degrees of obliquity will be sufficiently manifest from the construction.

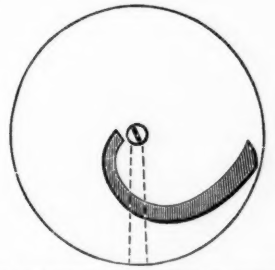


FIG. 2—PLAN OF UPPER AND LOWER PLATES MOVING FULLY ON EACH OTHER.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

To the Editor of "SCIENCE."

I do not like to see so great an authority as Faraday misunderstood, as he evidently is by your correspondent on page 459 of your journal, and that too, in a way which he took particular care to caution against—as to the law of gravitating action. That it acts inversely as the square of the distance he fully believed and admitted; or, to use his own words, "I know it is so."

If your correspondent finds difficulty to account by this law for the return of the earth from aphelion to perihelion, let him try to account for the return of, a stone to the earth when thrown up into the air; for precisely the same explanation applies to both, the highest point of the stone's path being "aphelion." The resistance of the air need not be regarded, for, though it modifies the stone's path, it does not affect the theory of the action of gravity.

GEO. B. MERRIMAN.

RUTGERS COLLEGE.

To the Editor of "SCIENCE:"

I have no desire to make any rejoinder to Dr. Rogers' reply (see SCIENCE, p. 459), but am willing to leave his answer with your readers just as he has given them.

I desire, however, to make the following corrections in my published letter:—

On p. 458, next to last paragraph, for "author of above question" read "author of above quotation." Same page, last paragraph, for "As to the law of inertia" read "As by the law of inertia." And on p. 459, last line of first paragraph, for "centrifugal" read "centripetal."

DES MOINES, *Sept.* 26, 1881. J. E. HENDRICKS.

BOOKS RECEIVED.

CELESTIAL OBJECTS FOR COMMON TELESCOPES, by the REV. T. W. WEBB, M. A., F. R. A. S.—Fourth Edition — Revised and greatly enlarged — The Industrial Publication Company, No. 14 Dey street, New York. Price $3.00.

From the number of inquiries we have received respecting the expected issue of a fourth edition, we believe it will be welcome intelligence to our readers, to learn that the work can now be obtained.

As the third edition was an enlargement of its predecessors, so the present and latest edition has been rewritten and again enlarged. Mr. Webb thus states his reasons for remodeling his work, and at the same time indicates many of the improvements that he has introduced.

"The unprecedented diffusion of a taste for astronomical observation during the last seven years has brought with it such a corresponding increase in the optical capacity of telescopes in private hands that the very title of this treatise would convey an inaccurate impression unless its contents were modified in accordance with the requirements of the time.

Without abandoning that elementary character which may still make it serviceable to beginners, its compass must now be greatly extended, if it may hope for acceptance as a manual by the more advanced student; and with this object, as the increase of telescopic range chiefly affects the sidereal portion, recourse has been had for additional Double Stars to the great catalogue of Struve I., as well as in a lesser degree to those of his son and Burnham, and as regards Nebulæ to that of Herschel II., with a total increase of about 1500 objects, some of which are chosen as tests worthy of the finest instruments, but occasionally, as is well known, within reach of those of more moderate dimensions."

The present edition of Mr. Webb's will soon find purchasers, and we advise all those who desire to possess a copy, to be prompt in securing it. The work is an indispensable manual to all who possess a telescope, or have a taste for astronomical studies.

A CORRECTION.—Professor Edward S. Morse desires to withdraw the first part of the last paragraph of the abstract of his paper on "Changes in Mya and Lunatia since the Deposition of the New England Shell Heaps," and substitute the following:—

"A comparison of the common beach cockle (Lunatia) from the shell heaps of Marblehead, Mass., showed that the present form living on the shore to-day had a more depressed spire than the ancient form; and this variation," etc., etc.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 8, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.
Sunday, 2	30.293	30.348	9 a. m.	30.196	12 p. m.
Monday, 3	29.961	30.196	0 a. m.	29.898	6 p. m.
Tuesday, 4	29.739	29.902	0 a. m.	29.632	3 p. m.
Wednesday, 5	30.135	30.268	12 p. m.	29.788	0 a. m.
Thursday, 6	30.246	30.350	9 a. m.	30.196	4 p. m.
Friday, 7	30.229	30.298	9 a. m.	30.188	4 p. m.
Saturday, 8	30.022	30.196	0 a. m.	29.894	12 p. m.

OCTOBER.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 2	66.3	62.0	75	0 a. m.	68	0 a. m.	63	11 p. m.	61	11 p. m.	92.
Monday, 3	74.3	68.3	82	4 p. m.	71	4 p. m.	63	0 a. m.	61	0 a. m.	137.
Tuesday, 4	67.3	59.3	77	3 p. m.	66	3 p. m.	50	12 p. m.	45	12 p. m.	131.
Wednesday, 5	40.0	35.6	46	4 p. m.	40	5 p. m.	35	8 a. m.	31	8 a. m.	110.
Thursday, 6	49.0	43.3	60	4 p. m.	50	4 p. m.	36	6 a. m.	35	7 a. m.	118.
Friday, 7	60.7	53.7	70	4 p. m.	59	3 p. m.	48	6 a. m.	46	6 a. m.	130.
Saturday, 8	69.6	61.3	80	4 p. m.	67	5 p. m.	59	7 a. m.	55	4 a. m.	134.

Mean for the week .... 30.089 inches.
Maximum for the week at 9 a. m., Oct. 6th .... 30.350 "
Minimum " at 3 p. m., Oct. 4th .... 29.632 "
Range .... .718 "

	Dry.		Wet.	
Mean for the week	61.0 degrees	....	54.5 degrees.	
Maximum for the week, at 4 pm. 3d	82. "	at 4 pm 3d,	71. "	
Minimum " " 8 am. 5th	35. "	at 8 pm 5th,	31. "	
Range " " ---- ----	47. "	----------	40. "	

OCTOBER.	WIND. Direction. 7 a. m.	Direction. 2 p. m.	Direction. 9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Force. Time.
Sunday, 2	n. e.	e. n. e.	e.	184	5	8.30 pm
Monday, 3	s. w.	w.	n. w.	124	5	3.50 pm
Tuesday, 4	w.	w. n. w.	n. n. w.	201	9	8.00 pm
Wednesday, 5	n. n. w.	n. n. w.	n. n. w.	377	12¾	4.40 am
Thursday, 6	n. w.	w. n. w.	w.	176	2¼	1.00 pm
Friday, 7	w. s. w.	s. w.	s. s. w	153	3½	1.00 pm
Saturday, 8	w. s. w	w. s. w.	w. s. w.	212	3½	2.30 pm

OCTOBER.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 2	.495	.502	.497	70	78	83	9 cu.	9 cu.	10	-----	-----	-----	--	0
Monday, 3	.577	.598	.666	84	58	77	10	4 cir. cu.	4 cu.	4.30 am 4.00 pm	9.30 am 4.20 pm	5.00 0.20	.26 .07	0 7
Tuesday, 4	.529	.490	.230	75	53	51	8 cu.	7 cu.	9 cu.	-----	-----	-----	--	3
Wednesday, 5	.142	.129	.190	70	44	74	0	0	0	-----	-----	-----	--	0
Thursday, 6	.199	.179	.256	90	40	61	0	0	0	-----	-----	-----	--	0
Friday, 7	.258	.290	.433	71	42	73	8 cu.	3 cir. cu.	0	-----	-----	-----	--	1
Saturday, 8	.409	.422	.476	82	45	59	3 cir. cu.	4 cir. cu.	4 cu.	-----	-----	-----		

Distance traveled during the week .... 1,427 miles.
Maximum force .... 12¾ lbs.

Total amount of water for the week .... .33 inch.
Duration of rain .... 5 hours, 20 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.